form of an animal is in great measure kept up by the same identical means by which a true *breed* is produced. The original form of a species is *unquestionably* better adapted to its *natural* habits than any modification of that form; and, as the sexual passions excite to rivalry and conflict, and the stronger must always prevail over the weaker, the latter, in a state of nature, is allowed but few opportunities of continuing its race. In a large herd of cattle, the strongest bull drives from him all the younger and weaker individuals of his own sex, and remains sole master of the herd; so that all the young which are produced must have had their origin from one which possessed the maximum of power and physical strength, and which, consequently, in the struggle for existence, was the best able to maintain his ground and defend himself from every enemy. In like manner, among animals which procure their food by means of their agility, strength, or delicacy of sense, the one best organised must always obtain the greatest quantity, and must, therefore, become physically the strongest, and be thus enabled, by routing its opponents, to transmit its superior qualities to a greater number of offspring. The same law, therefore, which was intended by Providence to keep up the typical qualities of a species, can be easily converted by man into a means of raising different varieties; but it is also clear that, if man did not keep up these breeds by regulating the sexual intercourse, they would all, naturally soon revert to the original type. Farther, it is only on this principle that we can satisfactorily account for the degenerating effects said to be produced by the much censured practice of breeding in and in.' There would almost seem, in some species, to be a tendency, in every separate family, to some particular kind of deviation, which is only counteracted by the various crossings which, in a state of nature, must take place, and by the above-mentioned law, which causes each race to be chiefly propagated by the most typical and perfect individuals" (pp. 45–46).[1]

On the suggestion of Prof. Cossar Ewart, the above quotation was submitted to Mr. Francis Darwin, who has kindly informed me that he agrees with my remarks in general, but is unable to state definitely the identity of the author.

In his introduction to the "Origin" Darwin notices several such "anticipations," but no reference is made to Blyth's name in this connection.

It seems indeed strange that Darwin should have been unacquainted with this article, and, what appears stranger still, that Blyth himself should have failed to direct attention to his paper, or that there should be no mention of these passages in either Darwin's or Blyth's correspondence. Mr. Francis Darwin has, however, indicated "More Letters," i., p. 62) that much of Darwin's correspondence with Blyth has not been forthcoming. This is to be regretted.

Curiously enough, in a letter to Lyell, Darwin says:—"Blyth says (and he is in many respects a good judge) that his ideas on species are quite revolutionised. . . ." ("Life and Letters," ii., 1887, p. 316.)

At this juncture the question naturally arises, viz., Is the Edward Blyth of the article *the* Edward Blyth of Calcutta? On turning to Grote's "memoir" (Journal Asiatic Soc. Bengal, August, 1875, part ii., supplement), we find (p. 5) that Blyth contributed to both Louden's and Charlesworth's series of the *Magazine of Natural History* from the year 1833. From the titles of the various articles which appear under Edward Blyth's name in Louden's Magazine, there is no evidence to indicate that all these contributions did not originate from the same writer. On Grote's evidence we are therefore justified in concluding that our author is the naturalist who afterwards made himself famous by his writings on, and profound knowledge of, the mammals and birds of India. Moreover, this conclusion is substantiated by our author's address, given in the same volume of Louden in several instances as "Tooting, Surrey," and we learn ("Dict. National Biog., London, 1886, vol. v., p. 276, art. Blyth, Edward) that Blyth purchased a druggist's business at Tooting on coming of age.

Mr. J. Ritchie, of the Royal Scottish Museum, has suggested to me that Blyth, in 1859, may quite easily have forgotten what he had written twenty-four years previously, the more so as he failed in the true application of his "principle." The association of his ideas with those of Darwin would, therefore, be incomplete or entirely wanting.

[1] The italics in this quotation are Blyth's.

Though Blyth seems clearly to have recognised the principle of natural selection, he fails in its true application in that he regards *his* "principle" as operating for the conservation rather than the progression of the type, whereas the two really go hand in hand, the one being a complement of the other in the successive stages of evolution. Moreover, proof of Blyth's inability to recognise the logical issue of his theory is exhibited in some of his remarks, which appear to disagree, or are incompatible with, one another. For instance, it is hard to reconcile the sentence commencing "Farther," and ending "breeding in and in," with some of his previous statements.

Blyth was a staunch supporter of Darwin's views, and his early theorisings are of interest in connection with his projeeted work on "The Origination of Species," which, however, was never completed, even in manuscript form (Grote, *loc. cit.*, p. xiv). H. M. VICKERS.

81A Princes Street, Edinburgh, February 3.

The Sailing-Flight of Birds.

IN NATURE of February 2, Mr. Mallock remarks that the skimming of some birds near the surface of the waves, where the variations in the velocity of the wind are great, may be dependent only on the inequalities of a horizontal breeze, and that an upward current is not absolutely necessary. My own observations have led me to the conclusion that whenever a bird glides for any distance without losing altitude he is, no less than the soaring kite or eagle, utilising an upward current of air. But it is possible that Mr. Mallock may be thinking of the albatross, who is perhaps without peer in his power of profiting by the vagaries of the wind. Unfortunately, I have had no opportunities of observing the albatross, and from those who have I get very conflicting accounts, some maintaining that he will glide for long distances under conditions which make it almost certain that the wind is horizontal, others holding that, though he brings the art to greater perfection, he does nothing different in kind from what the gull, that hangs with outstretched wings over the stern of a steamer, is able to achieve.

My object in writing this is to urge any of your readers whose good fortune gives them opportunities of watching the albatross on the wing to make careful observations on this very interesting subject. F. W. HEADLEY.

I AGREE with Mr. Headley that observations of the various conditions under which flight with fixed wings can be accomplished are desirable, but it is quite as important to determine the motion of the air in any particular case as to observe the behaviour of the bird.

In the case of a bird skimming close to the surface of waves, the action is presumably that sketched below. To

appreciate this properly, regard must be had to the vertical motion of the air in respect to time as well as to the wave surface. It is assumed that the speed of the wind is greater than that of the waves, and that the bird is flying to windwards. In these circumstances, the mean velocity of the air is less in the lee of each wave-crest than it is on the windward slope (indeed, when the waves are steep, the flow on the lee side may be reversed).

If a bird follows the course indicated by the dotted line, it gains, not only from the ascending current off the windward slopes, but also from the increased velocity it can acquire by dropping to a low level in the slower wind to the leeward of them.

The question of possible flight by variations of horizontal velocity has been treated by Lord Rayleigh and Mr. R. E. Froude. A. MALLOCK.

IN the flight of birds, besides the change in the inclination of the wing planes noted by the Rev. R. Abbav in NATURE of February 9, there must surely be some movement either of the wing, tail, or body which takes the place of the screw of the aëroplane. The seagull, for

instance, gives an occasional quivering motion to one or both wings which is clearly perceptible to the unaided eye, although propulsion and change of position relatively to air currents seem to be accomplished by strokes of the wings resembling sculling strokes.

It is not the birds, but certain insects, which exhibit quiverings of the wing imperceptible to the eye. The hoverer-fly, Syrphus, for example, can remain in one spot in the air while the wings are vibrating at such a rate as to be invisible, and at the approach of danger, or at will, it may suddenly by some movement, also invisible, transfer itself to a distance of a yard or more, and there continue the wing quiverings, which maintain the body almost stationary.

Is not motion in all flying and swimming things attained by presenting the wings or fins at a suitable angle to the air or water, while at the same time giving a propelling motion to the tail or dorsal fin and body, and also by a sculling motion of the wings or side fins, in the case of some insects and fishes, invisible to the human eye?

Derby, February 9. EDWARD D. HEARN.

Demonstration of Peltier and Thomson Effects.

THE following method of demonstrating the Peltier and Thomson effects may be of interest. In Fig. 1 the current passes through an *Sb*-B*i*-*Sb* bar, the points of contact being amalgamated to reduce the resistance. Two coils of No. 36 covered copper wire are wound on the bismuth

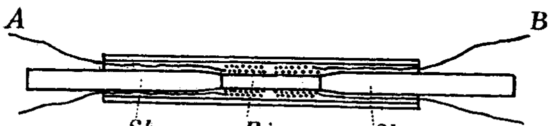

FIG. 1.

one near each junction, and by means of the leads A and B are placed in the gaps of a metre bridge, and a balance produced. On passing a current of 1 ampere through the bars, one junction is heated and the other cooled, which is indicated by a galvanometer deflection of about 40 mm. due to the change in resistance of the copper coils. The direction indicates a heating where the current flows from *Sb* to B*i*, and *vice versa*.

Fig. 2 shows a similar arrangement for exhibiting the Thomson effect. The bent iron rod is heated to red heat at C, and the ends A and B dip into vessels of mercury,

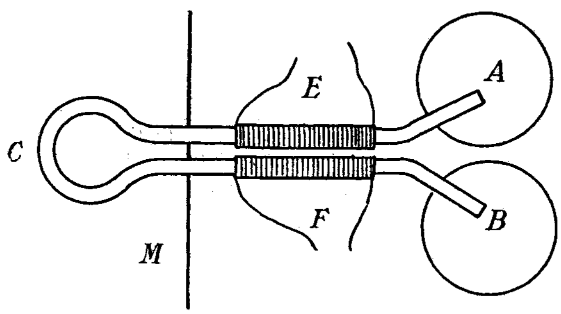

FIG. 2.

thus ensuring a large temperature gradient. On passing a current of 10 amperes in the direction ACB, AC is warmed and CB cooled, showing that the Thomson coefficient is negative. The part EF must be packed in asbestos wool to prevent heating disturbances from outside. S. G. STARLING.

Municipal Technical Institute, Romford Road, West Ham, E., January 28.

The Formation of Spheres of Liquids.

IN conducting Plateau's experiment for the formation of spheres of liquid in a medium of equal density, it is still customary to use oil of some kind in a mixture of alcohol and water. The following method will be found much simpler and more effective. A glass beaker about 10 cm. diameter and 15 cm. high is filled with water at 22° C. to two-thirds of its height. By means of a pipette, 100 c.c. of a solution of 30 grams of common salt in 1 litre of water are discharged at the bottom of the beaker, so as to form a lower layer slightly denser than the water above. A large funnel furnished with a tap, and having a stem 1 cm. or more in diameter, is now placed centrally in the beaker so that the stem terminates about 7 cm. from the bottom of the vessel. A quantity of commercial orthotoluidine, at a temperature less than 22°, is poured into the funnel, and the tap turned so as to allow the liquid to flow gradually into the water. A sphere of orthotoluidine forms on the end of the stem, the growth of which resembles that of a soap-bubble blown from a pipe.

It is quite easy in this way to make spheres 6 or 8 cm. in diameter, and the red colour of the orthotoluidine renders the procedure visible from a distance. The funnel may be lifted out and the sphere left floating in the water; and on surrounding the beaker by a square glass vessel, also containing water at 22°, the true spherical shape of the drop is seen. If the beaker be surrounded by cold water at 15°, the sphere will elongate in its horizontal diameter and sink, whereas if the surrounding water be at 27° or more, a vertical elongation will take place, and the sphere will rise and attach itself to the surface of the water in the form of a hanging drop. This behaviour is due to the fact that orthotoluidine and water are equal in density at 22°, but owing to the former liquid possessing a higher coefficient of expansion, it becomes less dense than water above 22°, and more dense at a lower temperature.

It may be added that all the usual experiments with liquid spheres can be carried out in the beaker, and the method of formation has the advantage that a sphere of any desired size may be formed by closing the tap when the requisite quantity of liquid has run out. In the course of a general investigation of liquids which are lighter or denser than water, according to temperature, the writer has found several which may be made to produce spheres at certain temperatures in the manner described, but has found orthotoluidine to be best suited to the experiment.

CHAS. R. DARLING.

City and Guilds Technical College, Finsbury, E.C.

Colliery Warnings.

I HAVE read the letters which have appeared on this subject with considerable interest. We have two theories before us. Both theories connect the presence of firedamp with changes of atmospheric pressure, but the one considers a time of high pressure as being most likely to cause an outrush of gas, whilst the other regards a falling barometer as the period of greatest danger. It does not seem at all reasonable to suppose that the atmospheric pressure would compress the rock and force out the gas as the Author of the Warnings suggests. Rather would air enter the rock cavities in such circumstances. The tendency for firedamp to escape during a falling barometer would be greater than during a rising barometer, but the evidence only shows a very slight connection to exist between the rise or fall of the barometer and colliery disasters.

The firedamp generated in certain coal measures exists in the rock, apparently, under considerable pressures, and its escape does not appear to be likely to be much affected by atmospheric pressure changes. The Author of the Warnings remarks:—"There was a time when no one guessed that the earth's surface was always on the move. . . ." In colliery districts the earth's crust is always on the move, owing to the colliery workings themselves. This movement is not a bodily oscillation—it is an actual rending of the strata for some distance below as well as above the seam being worked. Is it not likely that it is to the formation of fissures in the rock in this way that the gas owes its liberation? Considerable spaces may also be formed by the settling and creep in front of a working face; the firedamp would collect in such spaces and be forced out by further settling. At any rate, it seems clear that the escape of firedamp in quantity is more likely to be the result of some local change rather than to changes of atmospheric pressure.

R. M. DEELEY.

Inglewood, Longcroft Avenue, Harpenden, February 3.